riety of conditions to receive attention in ecology. Persons desiring further information may communicate with Dr. Charles T. Vorhies, University of Utah, Salt Lake City.

Mr. C. V. Hodgson, of the Coast and Geodetic Survey, Department of Commerce, has recently left Washington for the southwestern part of the United States, where he will have charge of a party for the determination of the astronomic latitude of triangulation stations established by the Coast and Geodetic Survey and the United States Geological Survey, between Barstow, Tex., and the Pacific Ocean. Many of these stations are on mountains as much as 10,000 feet in height. The results of this work will be used principally for geodetic purposes, that is, the determination of the figure of the earth and the distribution of material in the earth's crust. The means of transportation for this party will be a 1½-ton automobile truck which was used successfully on similar work between Denver, Col., and the Canadian border in the season of 1913. A similar truck had been employed also in 1912 on the 49th parallel boundary survey between the United States and Canada. The cost of the work during the season of 1913 is estimated as only one half what it would have been if horses and wagons had been used for transportation. The saving in the coming season is expected to be even greater, as the country to be traversed is arid or semiarid and the transportation of water and forage for stock would have been a difficult problem. It is expected that the work will continue until late in the autumn.

UNIVERSITY AND EDUCATIONAL NEWS

The new buildings of Zürich University were formally opened on April 19. The buildings, which cost about $1,700,000, stand on rising ground overlooking both lake and city.

Dr. Theodore C. Janeway, Bard professor of the practise of medicine in Columbia University, has accepted the professorship of medicine in Johns Hopkins University under the full time basis made possible by the gift of $1,500,000 by the General Education Board.

Dr. Mazyck P. Ravenel, professor of medical bacteriology in the University of Wisconsin, and director of the public health laboratory, has accepted the chair of preventive medicine in the University of Missouri.

The board of regents of the State University of Washington has appointed a committee of three to consider the selection of a president and has requested the faculty to choose a like committee, which it has done by secret ballot. This joint committee will report to the faculty and to the regents.

Dr. E. R. Clark, associate in anatomy at Johns Hopkins University, has accepted an appointment to the chair of anatomy in the University of Missouri.

Dr. Leverett D. Bristol has left Syracuse University to take charge of the North Dakota state laboratory.

At the College of the City of New York Professor William Fox has been made full professor of physics and head of the department and Professor Laurel has been promoted to be full professor of mathematics.

Dr. William Hallock Park, professor of bacteriology and hygiene in the New York University and Bellevue Hospital Medical College, has been elected dean of the college by the council of New York University to succeed the late Dr. Egbert le Fevre.

Dr. Albrecht Bethe, professor of physiology at Kiel, has accepted a call to Frankfort.

Professor Dr. H. Merkel, professor of pathological anatomy at Erlangen, has been called to Munich.

DISCUSSION AND CORRESPONDENCE

MULTIPLE FACTORS IN HEREDITY

In Science, April 10, 1914, Professor Ramaley refers appreciatingly to the work of Dr. MacDowell on size inheritance in rabbits, which was carried out in my laboratory, and concludes that this work essentially substantiates Davenport's conclusion that the apparent blend of human skin color in mulattos is due to two distinct Mendelizing factors possessed by the negro, but lacking in the white races.

Professor Ramaley's note might lead one to infer that since Dr. MacDowell's work was carried out under my supervision and since his paper has been published with my approval, therefore I (in common with Mendelians generally) share the views expressed concerning Mendelian factors in size inheritance, but this is not entirely true, and to avoid further possible misunderstanding I write this note. Dr. MacDowell's observations I believe to be accurate; they were made with great care and were checked in every possible way. I have kept in close touch with his work at every stage of its progress and have found it unimpeachable. Few investigators with whom I have been associated have shown such aptitude for exact and critical work as he displayed from the beginning. I endorse his observations fully.

But the facts observed are capable of different theoretical interpretations. In regard to these I have encouraged in Dr. MacDowell the fullest freedom of choice. He has adopted one for which much can be said, that of multiple Mendelian factors, which at times has appealed to me strongly, and the argument for which I have presented elsewhere ("Heredity," D. Appleton & Co., 1911) at some length. This theory has also been developed independently by Lang (1910), East (1910), Emerson (1910) and others. It accounts for the facts fully if certain basic assumptions are allowed, about which, however, I am growing more skeptical the more closely I examine them. Dr. MacDowell, in the passage quoted by Professor Ramaley, truthfully says of this theory, "It goes hand in hand with the mutation and pure-line doctrines of De Vries and Johannsen." But suppose one is not prepared to accept those doctrines, what then becomes of the multiple factor hypothesis? It is left without adequate basis. If the multiple factor hypothesis must stand or fall with the pure-line doctrine, I for one can not accept it, for the foundations of the pure-line doctrine appear to me very insecure.

What in brief are the facts regarding size inheritance which call for explanation? Fortunately, observers are quite in agreement concerning them.

1. Occasionally an unmistakable Mendelizing factor is concerned in size inheritance. One was discovered by Mendel himself (1866) and its existence has been repeatedly verified, namely, the differential factor between tall and dwarf races of garden-peas. Tall and dwarf conditions in other plants behave in a similar way, that is as Mendelian alternative conditions showing both dominance and segregation in crosses. Brachydactyly in man is a variation like dwarfness in plants, in which the growth habit is altered, the skeleton being abnormally short and compact throughout. This character is a Mendelian dominant (Farabee, 1905, Drinkwater, 1908). The shorter, more compact form of Dexter cattle, in contrast with the Kerry breed, is a Mendelian character (Jas. Wilson, 1909) probably similar in nature. Doubtless the same was true of the short-legged Ancon sheep mentioned by Darwin (1878, "Animals and Plants").

From the mere fact that a Mendelian factor may be involved in a size difference, it by no means follows that *all* size differences are due to Mendelian factors. Such Mendelizing factors affecting size as have just been enumerated are distinctly rare. They are not discoverable at all in the cases studied by MacDowell, which involve neither dominance nor segregation in a 1:2:1 ratio. Even in cases involving an unmistakable Mendelian factor, as the tall-dwarf cross in peas, it is not to be supposed that no other factors affect size. For are all dwarf peas of the *same* height, or are all tall peas of the same height? No, there are differences among each sort, differences which are heritable also, since one dwarf variety differs from another in its mean height.

2. Ordinary differences in size (such as do not involve a change in the growth habit) among animals or plants do not Mendelize in the ordinary acceptation of the term. When races are crossed which differ widely in size, the first filial (F_1) generation is intermediate between the parents and often not more variable than one of the parent races. But the

second filial (F_2) generation, though still intermediate, commonly shows increased variability, the range of which may even extend into or include the size range of one or both parent races. This *increased variability* of the F_2 generation is the only evidence of Mendelism in size crosses. In 1911 I was inclined to regard it as *sufficient* evidence, but in this I was clearly mistaken, as a moment's consideration will show. It would be sufficient only (1) if the size differences were due *wholly* to Mendelian factors, and (2) further these factors were invariable, that is quantitatively always the same. But neither of these assumptions can be regarded as established. On any hypothesis size differences must depend on many mutually independent factors or causes. This is the prime significance of a frequency-of-error variation curve, however produced. It would be rash to assume that all the factors concerned are *Mendelizing* factors, in the total absence of the two usual accompaniments and criteria of Mendelism, dominance and segregation in recognizable Mendelian ratios.

The question whether Mendelian factors are constant or inconstant has been discussed from different points of view by my colleague Dr. East and myself in the *American Naturalist* (1912), he maintaining their constancy on the ground that they are subjective merely, while I have thought it necessary to assume for them an objective existence in the germ-cell, and am unable to discover any evidence of their constancy from the behavior of germ-cells. It is, of course, possible, as Dr. East maintained, to formulate a description of all heredity in terms of (purely subjective) Mendelian units, provided more and more units are from time to time created (by imagination) as the objective facts show the organism changed. But such an extension of Mendelism fails to interest me, as I think it does many of my readers. What we want to get at, if possible, is the objective difference between one germ-cell and another, as evidenced by its effect upon the zygote, and it is the constancy or inconstancy of these objective differences that I am discussing. If these are quantitatively changeable from generation to generation, then change in the variability of the zygotes composing a generation might arise without factorial recombination.

By way of illustration let us consider the simplest conceivable case. Suppose two organisms to differ by a single genetic factor for size. Suppose one organism to be of size 4, the other of size 8. On crossing, if each transmits its own condition and dominance is lacking, an intermediate is formed, size 6. On the theory of gametic purity, the gametes formed by this heterozygote of intermediate size (6) should be 4 and 8, respectively, and the next generation of zygotes (F_2) should be as follows:

Classes of zygotes	4,	6,	8,
Expected frequencies	1,	2,	1.

Here we note that a large part of the F_2 generation is intermediate in character, as was F_1, but F_2 is more variable than F_1, falling into three classes instead of one. This is the regular Mendelian way of viewing size inheritance, gametic purity being assumed. But is the assumption necessary or justifiable? Suppose the assumed size factor were *modifiable* or partially blending, so that 4 and 8, after association in the F_1 zygote, emerged as 5 and 7, respectively, in the gametes. Then F_2 would be:

Classes of zygotes	5,	6,	7,
Expected frequencies	1,	2,	1.

In this case, as well as in the supposed case of pure gametes, we should observe an F_2 more variable than F_1, though the *extreme* conditions of the parent organisms crossed would not be attained in F_2 (as, in fact, they rarely are). But even the recurrence of such extreme conditions as those of the grandparents might be explained as due to occasional failure of the gametes associated together in F_1 to modify each other.

Now I do not advocate either of these explanations. I present the second merely to show that the first is not the only conceivable explanation, and that I was earlier wrong in supposing an *increase* of variability *prima facie* evidence of the occurrence of *more* than a single Mendelizing factor. It might equally

well be regarded as evidence of a *single* Mendelizing factor, quantitatively variable.

If we set out by assuming that a Mendelian factor is invariable, then we are forced to assume, whenever genetic variation is observed in an organism, that this is due to an *additional* Mendelian factor. This is the real basis of the multiple factor hypothesis as applied to size inheritance, though not, of course, the historical one. But to reason thus is merely to pile one assumption upon another, which is not to advance science, whatever it does for a system or a terminology; but with these we are less concerned than with knowing the exact truth and in stating it as clearly and concisely as possible.

What now of human skin color, is this or is it not Mendelian in inheritance? At present I consider this largely a question of terminology. The *facts* appear to be very similar to those observed for body-size in rabbits, and for other quantitative characters in animals and plants. F_1 is intermediate; F_2 is also intermediate, but more variable than F_1. If we call this Mendelism, we shall need to explain that it is not the Mendelism of Mendel himself, but original Mendelism *plus* (1) the assumption of gametic purity, *plus* (2) the assumption of factorial constancy, *plus* (3) the assumption of factorial multiplicity.

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JAVEL WATER—A SIMPLIFIED AND CORRECTED SPELLING

IN his "Grand dictionnaire universel du XIXé Siécle" (Paris, 1873), Pierre Larousse tells us that there used to stand upon the banks of the Seine in the suburbs of Paris in what is now the "XVe arrondissement" a solitary mill, sheltered by trees, where bathers and fishermen used to rest and partake of refreshments. This mill was known as the "moulin de Javel" and the lexicographer elsewhere states that this word is a variant of *javeau,* which means an island of sand and mud, a sandbank, although in this instance it is no doubt a proper name. The word *javelle* (cf. English, provincial, gavel), signifying an unbound sheaf or a bundle of grain smaller than a sheaf, is of different origin.

Upon the site of the old mill the village of Javel was founded in 1777 by the Count of Artois, who established a chemical works. The first directors of the works, Messrs. Alban and Vallet, were the originators of Javel water, which they prepared in 1792 by passing a current of chlorine through a solution of 2.440 kilos of "sub-carbonate" of potassium in 17 kilos of water. Larousse also refers explicitly to the erroneous spelling *eau de javelle,* employed by some authors. Littré in his "Dictionnaire de la langue française" (Paris, 1873) employs this erroneous spelling.

Unfortunately, the dictionary of the French Academy (7th edition, Paris, 1878) sanctioned the spelling "Javelle" for both the name of the mill and the derived name of the bleaching liquor, adding error to error in describing the liquid as a solution of potassium *chloride* in water ("L'eau de javelle est du chlorure de potassium en dissolution dans l'eau").

With such authority behind it, it is not surprising that the *-elle* ending has come into very general use. Yet a number of the more careful French and English writers employ the correct form of the word. Among such are Girard in "La grande encyclopédie" (article "Chlorures décolorants"); Emile Bouant in his "Dictionnaire de chimie" (Paris, 1888); Moissan in his "Chimie minérale" (1904–6); Edmund Knecht in the Encyclopedia Britannica, eleventh edition (article "Bleaching"); and Sir Edward Thorpe in his "Dictionary of Applied Chemistry" (1912). In Germany and America, as far as I have observed, the erroneous spelling is universally adopted. And our dictionaries of the English language appear likewise to be unanimously wrong. Not even the New Standard Dictionary (1913), which gives the simplified spellings of the Carnegie board, makes the least reference to the shorter form of this word.

A certain amount of confusion is prevalent also regarding the signification of the term